and rarest distinction for State service which now exists, and it is an honour to the few men who hold it that a woman, Miss Nightingale, belongs to the Order.

With regard to his present Majesty, King George the Fifth, to judge of what he has done as Prince of Wales, we may feel sure that there will be no falling off from the strenuous life of public duty exhibited by his father and grandfather.

We are informed by those who have had the privilege of conversing with him that, as one of the effects of his education as a naval officer, for the education of a naval officer must be scientific, he is fully in sympathy with those who insist upon the importance of science as one of the foundations of the greatness of a nation. It was this interest in science which caused him to regret deeply his inability to accept the presidency of the British Association at its meeting in South Africa. State duties had called him to India at that time.

King George is unapproached as a traveller, among crowned heads, whether of his own or of previous time. The new sovereign has probably seen more of the Empire than has been traversed by any single person among all his subjects. After the Australian Commonwealth was inaugurated, the King and Queen journeyed fifty thousand miles by land and water. They returned to Canada for the great tercentenary pageant at Quebec and the consecration of the battlefields, and meanwhile, in 1905, they had put a girdle round the globe again and traversed India from the Khyber to the Ganges, and from the Himalayas to the temples of the South. It has been well said that not one of his subjects realises better than the King how little is known of England by those that only England know, or how inseparably the whole life and fortunes of this island are knitted up with the destinies of the outer Empire. That many of the national shortcomings to which it is our duty to refer from time to time are fully known to his present Majesty is proved by his famous warning, uttered after all his journeyings, "Wake up, England."

In the midst of the nation's grief at a loss so great and so sudden that there has not been time to realise it, comfort may be found in the assurance which we possess that in the future, as in the past, the illustrious Royal Family will guard the best interests of our Empire. They will thus retain the affection and respect of a grateful people, whose hearts now go out to them in profound sympathy with their sorrows.

EDITOR.

THE NEW "ORIGIN OF SPECIES."

The Mutation Theory. Experiments and Observations on the Origin of Species in the Vegetable Kingdom. By Hugo de Vries, Professor of Botany at Amsterdam. Translated by Prof. J. B. Farmer and A. D. Darbishire. Vol. i., The Origin of Species by Mutation. Pp. xv+582. (London: Kegan Paul and Co., Ltd., 1910.) Price 18*s*. net.

THE two smaller books by Prof. de Vries written in English have already put us in possession of a clear account of his theory of the origin of species by discontinuous variation. The publication of a translation (of which this is the first volume) of his larger "Die Mutationstheorie" will be valuable to students as containing the detailed arguments upon which it is based.

De Vries has long been known as a distinguished physiologist who has the advantage of a sound training as a physicist. Now a physicist, in attacking a problem, endeavours to get it into the simplest form which admits of experiment and measurement. Having obtained a "law" which fits his results, he proceeds to test its application to more complicated phenomena. But "life" is a complex which at present defies analysis, and the naturalist is therefore obliged to survey organised nature as a whole, and to endeavour to extract from its observation principles of general application. In this way organic evolution has been arrived at, for it is now generally admitted to find its securest basis in the evidence afforded by palæontology of the succession of living forms in the past history of the earth. It might also, though less convincingly, find it in the facts of geographical distribution. Darwin did not invent evolution, but he made it immensely more acceptable, first by breaking down the old belief in the constancy of species, and, secondly, by giving, in "selection," a mechanical explanation of how they came into existence.

To this de Vries raises the same objection as Huxley, though on different grounds, that "we do not observe actual specific changes in nature" ("Plant Breeding," p. 4). His object is to overcome this difficulty. He agrees (p. 207) that

> "we have a doctrine of descent resting on a morphological foundation. The time has come to erect one on an experimental basis."

In this he believes he has succeeded. Every-one must admire the patience and enthusiasm which he has brought to the task, and if the naturalist must be permitted to criticise the result from his point of view, he cannot but approve the scrupulously fair and instructive way in which de Vries has published every detail of his research.

In order to do justice to the theory which de Vries has founded on his observations, it is necessary to attach some precise meaning to the technical terms used. Darwin was nothing if not a naturalist. It is impossible to read de Vries's pages without feeling that he is not quite at his ease with the naturalist's technique. On p. 21 I find, for example, the following statement:—

> "It is most remarkable that in the 'Index Kewensis,' which was published at Darwin's expense after his death, no distinction is drawn between varieties and synonyms."

I do not suppose it was intended, but this might be construed to mean that the work as executed failed to carry out Darwin's intention. That this would be the reverse of the fact I can state with confidence, as the plan was discussed and settled between Mr. Darwin and myself in his own house. What he told me he wanted was to be able to trace to its source any specific name that he met with. This the "Index" does, and it is an incalculable boon to those who use it. But its purpose is merely bibliographical, and it was not intended to express any opinion as to the validity of the species which it catalogues; nor was there ever any suggestion that it should catalogue varietal names.

It would be quite possible for a discussion of de Vries's theory to evaporate into a mere logomachy, a mere dispute as to the meaning of words. That I hope to avoid, but if I fail, he himself will have to share the blame. The word "mutability," which is peculiarly de Vries's property, and, indeed, contains the kernel of his theory, is an example. So far as I can find, it does not occur in Darwin's writings. Yet on p. 202 I find the following summary of the Darwinian theory:—

> "Mutability may take place in almost all directions; and it is natural selection which operates in one direction during long geological periods";

and that there may be no mistake, he adds that this "obviously represents the view of Darwin." This is emphasised on p. 198, where "species-forming variability" and "mutability" are treated as synonyms. But de Vries's whole contention is that, in the ordinary acceptance of the terms, they are profoundly different. This is apparent from the following passage in the author's "Plant Breeding" (pp. 5-6), a book which, I confess, seems to me to be written with more precision that the larger one.

> "The phenomena that follow Quetelet's law are now considered as one group, which is called fluctuating variability or fluctuation, since the individual qualities fluctuate around their average. The processes by which new qualities are produced must be studied separately. Under the assumption that these processes are neither slow nor invisible, but consist in leaps and jumps such as are popularly indicated by the name of sports, they are now called mutations, and the great subdivision of the phenomena of variability is designated, in consequence thereof, as mutability."

Now there is no want of precision about this statement. It of course assumes the very point which has to be proved, that "fluctuating variability" does not produce "new qualities"; but that is another story.

De Vries has elsewhere employed the words continuous and discontinuous, as descriptive of the two processes. Unfortunately, Bateson used these at the same time in a different sense, and this has added another pitfall to the discussion of the subject. At first sight we seem to have a sharp distinction between the theories of Darwin and of de Vries, the one accounting for the origin of species by the accumulation of small variations, the other by "sports." If we take, as an example of the latter, the production of the nectarine from the peach, there can be no doubt that the leap may be considerable. While the occurrence of sports is undeniable, de Vries appears to draw a distinction between the part attributed to them by Darwin and Wallace in the production of species (pp. 12 and 39). Wallace regards them "as absolutely without significance" (p. 40). But Darwin, with more caution, doubts whether they "are ever permanently propagated in a state of nature" ("Origin," fifth edition, p. 49), and this is practically the same thing. De Vries himself is led gradually to minimise their magnitude. He finds (p. 53) "sports" "not a happy" term, and prefers "jerky variability" where "jerks may only induce quite small changes." Later on he finds (p. 55) that "many mutations are smaller than the differences between extreme variants." He quotes Galton's polyhedron which, when disturbed, "oscillates round its position of equilibrium" (fluctuation), and finally "comes to lie on a new side" (mutation). But when the faces are very small, the illustration is obviously not helpful; when the variation is very small, the distinction between the two kinds is inappreciable.

To meet this difficulty, de Vries sets up other criteria. These he has drawn from a mass of experimental work for which it is impossible to conceal one's admiration, and is a positive addition to our knowledge. But it is, of course, permissible to draw from it somewhat different conclusions. "Mutants" arise without transitional forms (p. 248). It appears, however (p. 504), that such do occur, but simultaneously, and not before; the mutants, therefore, are artificially selected from a varying population. But they possess from the first absolute stability; this is an extremely interesting point; it is, of course, implied that they were self-fertilised, and that the conditions were unchanged. It is, however, to be noticed that it was not universally the case, as three of the mutants studied "proved to be inconstant," and some actually reverted to the original parent (p. 508). Such mutants he calls "elementary species," a term for which he invokes (p. 57) the authority of Darwin, who, however, only said that "varieties are incipient species," a very different thing.

It follows that "specific characters are absolutely independent of selection" (p. 90); "by natural selection species are not *created* [no one ever thought they were], but eliminated" (p. 199). Selection "now has served its time as an argument for the 'Theory of Descent'" (p. 29). It was, indeed, finally disposed of by Lord Salisbury (p. 70), a fact which would have more weight if he had not ludicrouslv misapprehended the Darwinian theory.

The utility of specific characters (p. 65) and adaptation necessarily go by the board as well. Desert plants are not adapted to desert life. "They endure the desert, but only with difficulty" ("Plant Breeding," p. 350); "the spurs of the orchids . . . have not been originated in the way in which plants are now using them" (*l.c.*, p. 352). It must be admitted that all this is perfectly consistent, and follows logically from the conception of the species as an arbitrary result, "independent of the environment," and for which "we can as yet assign no cause" (p. 130). This makes a pretty complete sweep of the Darwinian theory, and practically takes us back to the position of Linnæus, who was content to suppose that species were created. It must, however, be put to de Vries's credit that he makes an heroic attempt to save Darwin himself from the wreck by claiming him as at heart a mutationist (p. 87) who "only by the pressure of criticism" (p. 39) gave up the true faith. There is a little irony in the fact that, as the critic was apparently Fleeming Jenkin (p. 37), the mischief was the work of a physicist. The attempt, though generous, is scarcely convincing, and so Darwin goes, and with him goes the splendid and fertile field of biological research for which he opened to us the door. Its place is taken by the procession of arbitrary mutants which have nothing to tell us because they have nothing to say. It is a rather dreary outlook, only mitigated by the "hope that we may be able to gain some control over the formation of species" (p. 186). Yet when I turn to Darwin and read "We have no evidence of the appearance, or at least of the continued procreation, under nature, of abrupt modifications of structure" ("Variation," ii., p. 414), I pluck up a little conviction of something that seems more full-bodied than a mere echo of Fleeming Jenkin. It may be remarked, however, that the celebrated argument of the latter has never been refuted.

I do not know whether to take comfort from the fact that if Darwin fares badly, Lamarck fares worse.

> "Specific characters are never 'acquired'; and there is no need for taking 'acquired characters' into consideration in the whole domain of comparative biology and the theory of descent" (pp. 130–1).

That gets a troublesome question out of the way, at any rate. To finish clearing the ground, a last dictum :—"No theory of the origin of species can have any bearing at all on this subject" [sociology] (p. 159). "Man is immutable albeit highly variable" (p. 156), a reservation with little satisfaction, as de Vries will allow no stability to racial variation.

It may be asked, on what are these sweeping conclusions based? Bateson inferred the discontinuity of variation from the observed discontinuity of species. De Vries apparently does not accept this, but for once adopts the obvious Darwinian explanation, "Subspecies become species by the extinction of intermediate forms" (p. 186). His own theory is based on the prolonged study of a single species of unknown and suspect history. It is, perhaps, one of the most remarkable cases in science of generalisation from a single instance, and that highly dubious.

He commences the account of his experimental work by remarking (p. 217) :—

> "The chief obstacle in the way of . . . investigating the origin of species is our complete ignorance of the conditions under which the process takes place."

To test it, he "brought over one hundred species into cultivation" from the neighbourhood of Amsterdam. "Only one of these turned out to be what I really wanted." He does not tell us in what respect they failed, but concludes that mutability occurs "relatively rarely." It seems, therefore, to be open to the same kind of objection as has been urged against natural selection, that it "will generally act," as Darwin said, "very slowly, only at long intervals of time" ("Origin," sixth edition, p. 85). I think it is clear that what baffled de Vries was specific stability. It takes a somewhat prolonged subjection to cultural, *i.e.* changed, conditions to break this down. I have given numerous instances in these pages of extremely stable species which ultimately broke down. I have watched the process at Kew in *Primula obconica,* which now produces flowers two inches across. The process was described by Vilmorin half a century ago :—"The fixed character of the species being once broken, the desired variation will sooner or later appear (Darwin, "Variation," ii., p. 262). I cannot doubt, therefore, that if de Vries had had patience, every one of his hundred species would have become plastic in his hands.

He, then, has been obliged to rest his theory on a single case, that of *Œnothera lamarckiana.* There is no evidence that this is a "natural species." It made its appearance in the Paris Jardin des Plantes, and, as pointed out by Lock and Boulenger, de Vries's results are open to the interpretation of being simply due to "Mendelian disjunction."

But, waiving this point, there is a graver difficulty. De Vries insists (p. 130) that his mutations are "independent of the environment." A moment's reflection will show the disadvantage under which his theory rests in this respect compared with that of Darwin, which provides a means of automatic and continuous equilibrium, and remorselessly eliminates whatever fails to adjust itself to it. De Vries admits (p. 199) that his mutations must submit themselves to selection; but they are so handicapped at the start that they can rarely have much chance, and this is confirmed by the inability of sports to hold their own in nature. Even *Œnothera lamarckiana,* whatever it may do under cultivation, does not appear to be able to produce permanent varieties under natural conditions.

Two other lines of argument require brief examination. De Vries points to the familiar little crucifer, *Draba verna,* which is now in flower everywhere, as a case of natural mutation. Jordan collected from different countries some two hundred forms, differing often in extremely minute characters, but which he and other observers found to be stable in cultivation. Most systematists regard them as mere varieties. But species are only the stable forms of diverging races, the connecting links between which have disappeared. A naturalist will unite in one species in the Linnean sense, as in the case of *Draba verna,* a range of forms where it is conceivable that the differences could be bridged by continuous variation. Darwin thought that such forms might be stable, as, indeed, they appear to be. They may, in fact, be regarded as adjusted to slight differences in the environment, a view which derives some confirmation from the fact, which I confess surprised me, that de Vries finds only one of the two hundred forms of *Draba verna* in the neighbourhood of Amsterdam. This view would, of course, be rejected by de Vries, who looks upon them by analogy as mutants. The Darwinian explanation equally rests on an analogy, the production of the races of cultivated plants by artificial selection.

De Vries feels the force of this, and it is therefore essential to his case to break down the analogy. He admits (p. x) that "the process of selection has enabled us to produce improved races." But he draws a curious distinction between horticultural and agricultural methods. "In horticulture, varieties arise by mutations. . . . In agriculture, the highly improved races arise gradually through selection" (p. 82). For my part I am quite prepared to admit that mutation has had some share in the production of the latter; and as to the former, de Vries quotes my own observations on *Cyclamen latifolium,* where there can be no doubt that the existing race is the result of continued selection. The distinction, in fact, cannot be sustained.

De Vries objects to any argument drawn from cultivated races. We know little usually of their origin (pp. 13, 14), and this is true; they are often of hybrid origin (p. 75), which is equally true; both objections, it may be noticed, have been urged against his own Œnothera; finally, "improved races" (unless arising by mutation) lack stability (p. 120). This has been fully discussed by Darwin, and both he and Sir Joseph Hooker insist "on what little evidence this belief rests" ("Variation," ii., p. 32). Such races are the result of artificial selection, and are only stable under artificial cultural conditions. If these are withdrawn a new adjustment has to be sought, and, as Wallace has pointed out, in the face of competition for which they are in no way fitted. As Darwin says, "to assert that we could not breed our . . . esculent vegetables, for an unlimited number of generations, would be opposed to all experience" ("Origin," fifth edition, p. 16). The fact is accepted in the law courts, and if a pea such as Veitch's Perfection did not come true from seed, the purchaser would have a cause of action against the vendor.

De Vries states (p. 124) that "fruit-trees grown from seed quickly revert to the original type." This is too extreme a statement. The measure of truth in it is easily accounted for. Stability can be obtained in races where seminal reproduction can be quickly repeated. But a generation of fruit trees approximates to that of mankind, and though, theoretically, stability might be obtained, it would require a Methuselah to do it.

De Vries prefaced the first edition of this book by remarking that "The origin of species has so far been the object of comparative study only." It must always be to his credit that he has been the first to submit it to experiment. If the results seem so far inconclusive, the method, in the long run, must be fruitful. Darwin deliberately relied on continuous variation, de Vries relies on discontinuous. It must be obvious that the former has been a potent solvent of a wide range of biological problems where de Vries leaves us without an answer; and he frankly admits that on the fundamental principle we are as much in the dark as ever. For mutations, "we can as yet assign no cause" (p. 130), though (p. 207) he suggests that "the opportunity for the appearance of mutations is at once given" by change of environment. Beyond this we are no wiser as regards one kind of variation than as regards the other. We are evidently a long way from that "control of the mutative process" which "will, it is hoped, place in our hands the power of originating permanently improved species" (p. x). W. T. THISELTON-DYER.

PHYSICAL SCIENCE IN THE TIME OF NERO.

Physical Science in the Time of Nero; being a Translation of the "Quaestiones Naturales" of Seneca. By J. Clarke, with notes on the treatise by Sir Archibald Geikie, K.C.B., P.R.S. Pp. liv+368. (London: Macmillan and Co., Ltd., 1910.) Price 10*s*. net.

THE genius of the Roman people was mainly for action, conquest, and organisation. In the realms of thought they made but few original advances, here showing a striking contrast to the Greeks, whose progress in civilisation some few centuries previously had been intellectual rather than material, and made in art, philosophy, and speculation as to the deeper problems of life and nature. Of course, western nations owe, and have owed, an immense debt to the intellectual advances made by the Greeks, and there seems little danger that this debt will ever be underestimated. But there is reason to fear that full justice may not be done to the scientific progress made by the Romans; and Mr. Clarke's admirable translation of Seneca's "Quæstiones Naturales" comes very opportunely to illustrate its extent.

As Mr. Clarke points out in his introduction, Lucius Annæus Seneca was an exceptionally able man of many aptitudes—rhetorician, advocate, philosopher, author, politician, and for fifteen years tutor first and latterly also *âme damnée* to that monster of iniquity, the Emperor Nero. During his lifetime Seneca must have been best known for his eminence as a politician; during the many years when he was out of imperial favour, and had no share in politics, he devoted himself to philosophy, science, and authorship. As a philosopher he illustrates the eclecticism